

of which document will always be a disputed point with entomologists, inasmuch as (although twice quoted by its author in his subsequent works) it is more than doubtful whether it ever was actually published.

The succeeding chapters are devoted to the differential characters of the family, to structural details, habits, development, secondary sexual characters, origin of the genera and species, and mimicry; then follow concise descriptions of the genera and species, with comparative and other valuable notes, descriptions of preparatory stages, &c.

It is a subject for congratulation, the importance of which none but the working lepidopterist can fully appreciate, that Dr. Packard has devoted six of the plates to the delineation of wing structure; most of the generic errors in Mr. Walker's lists must be attributed to his entire neglect of the characters offered by neurulation; attention to this is sometimes the only means by which species, otherwise wholly similar, can be distinguished. The structure of the thorax, although of much importance, can rarely be attended to, as the destruction of the specimens is necessary before it can be detected; but in the examination of the wing-veins nothing is needed but a bottle of benzine, a brush, and a pocket lens, to reveal all that is required without injury to the insect.

In conclusion we heartily congratulate Dr. Packard on having produced a work in every respect worthy of himself and the Academy of which he is an officer.

A. G. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Prof. Balfour Stewart on Meteorological Research

IT occurs to me to make the following remarks with reference to Prof. Balfour Stewart's proposal in NATURE, vol. xiv. p. 388.

I cannot see any objection either to the nomination of the council which is suggested, or to its constitution, provided each existing society is duly represented by a member who can, when circumstances seem to require it, attend and vote at any meeting of the Council in London. I quite agree with the Professor in thinking that the time has now come when our country should resolutely grapple with the data which have accumulated in past years and with those that are now being obtained. It is only by a thorough discussion of meteorological data that the importance of certain principles can be detected, and the necessity for altering the modes of observing can be demonstrated.

I do not see that the appointment of the proposed council should interfere prejudicially with the working of the different societies. While it is the duty of such societies to procure the facts, it seems to me essential in order to secure uniformity in instrumental observation, without which all deductions or generalisations from the data may be worse than useless, that a council of control should be appointed in order to lay down rules for regulating all observers. I would not give an arbitrary power to that council to compel every society to adopt their views, because I have a great aversion to centralisation in matters of science, for in some cases the branches may be more in the right than the head; but in the event of a society declining to comply with the rules issued by the council, that society should not receive Government aid excepting for work that is done in terms of the rules. I take it for granted, however, that the council would give due weight to the arguments which were adduced from time to time by the representatives of the different societies. I am further of opinion that the different bodies should not only be allowed but encouraged by Government aid to prosecute independently in their own way, any special subject which they may choose to take up.

I hold so very strongly the absolute necessity of uniformity in instrumental observation, that I should be disposed to recommend each society to adopt almost any change in the forms of instruments, in the kind of exposure, in the hours of observation, in the form of protecting boxes, or in any other matter which might be recommended by the proposed council, provided such

changes were practicable, and were agreed to once for all by the other societies in this country, and by foreign nations.

I think it right to add that I am only stating my own individual convictions, and do not in any way profess to represent the opinions of the Council of the Society of which I am the honorary secretary, although I have no reason to suppose that they would take a different view.

THOMAS STEVENSON

Edinburgh, November 18

Ocean Currents

In the report published in NATURE (vol. xiv., p. 492) of an address given at the Glasgow meeting of the British Association, September 11, by Sir C. Wyville Thomson, and revised by the author, the following passage occurs:—

"We have come to the conclusion that this great mass of water is moving from the Southern Sea, and there seems to me to be very little doubt—although this matter will be required to be gone into carefully—that the reason why this water is moving from the Southern Sea in a body in this way, is that there is a greater amount of evaporation in the North Atlantic and over the northern hemisphere generally, than there is of precipitation, whereas it seems almost obvious that in the southern hemisphere in the huge band of barometric low pressure round the south pole, the precipitation is in excess of the evaporation."

Now I quite feel that I am guilty of very great presumption in challenging in any way the theories of so great an authority as Sir C. W. Thomson, and my only excuse for the remarks I am about to make is that there are some points that I and many other seamen would like to have cleared up before we entertain such an hypothesis.

1. Have the investigations of the *Challenger* sufficiently proved that there is no compensating or return current from the North Atlantic to the South Atlantic Ocean? Especially, is it quite certain that a stream of water from the Arctic regions does not set southerly along the West Coast of Africa, i.e., south of the equator?

2. Allowing that the precipitation in the Antarctic regions is greatly in excess of that in the Arctic regions, is the precipitation in the north torrid and north temperate zones less than the precipitation in the south torrid and south temperate zones?

3. Looking to the much larger distribution of land in the northern hemisphere, is it likely that the evaporation there is in excess of the evaporation in the southern hemisphere?

4. Even supposing the evaporation in the northern hemisphere to be in excess of that in the southern hemisphere, can it be shown that this vapour is carried to the Antarctic regions for condensation, or can the excess of precipitation in the Antarctic regions be accounted for in a more probable manner?

In answer to the first question I can only say that I am not able to gather from the reports of the ocean soundings and temperatures of H.M.S. *Challenger*, published by the Admiralty,¹ that it has been at all proved that there is no compensating stream of Arctic or other water.

In answer to the second question, I have never heard it disputed, and my experience as a seaman leads me to doubt the possibility of reasonably disputing, that the rainfall in the north temperate and north torrid zones is not only not less, but that it is far in excess of the rainfall in the south torrid and south temperate zones. Maury (and no matter to what extent we may differ from his theories, we must give due weight to his data) says that the total amount of rain in the north temperate zone is half as much again as in the south temperate zone.

With reference to the third question, whether the evaporation in the northern hemisphere is in excess of that in the southern hemisphere, I think the onus of proof rests with those who start the theory, but in my present state of ignorance on this subject I must confess that it is to my mind quite inconceivable. There are, with few exceptions, no large rivers in the southern hemisphere, and surely the discharge into the sea of the large rivers in the northern hemisphere must be regarded as the return to the ocean of the excess of precipitation over evaporation in the regions which they drain.

There remains the fourth question, and before trying to answer this I should like briefly to state what I think is the general or accepted belief up to the present time with reference to atmospheric currents or circulation. The trade winds are supposed to be currents from the poles which, starting from the Polar

¹ Plate VI. Report No. 7 would appear to indicate that Arctic water does cross the equator.

regions as upper currents, descend to the surface of the globe on the equatorial side of 30° of lat. in both hemispheres, they then travel onwards towards the belt of equatorial calms, when they meet and ascend into the upper regions of the atmosphere, whence they travel back towards the poles as upper currents, until they arrive at the calm belts of Cancer and of Capricorn, on the polar sides of which they once more descend to the surface, and are then known as the westerly winds of the temperate zones. Owing to the rotatory motion of the earth, it is impossible for these westerly winds to blow direct towards the poles, but it is clear that if you surround the Polar regions with a belt of westerly winds, that no matter what the direction of the wind may be in the Polar regions, it must, if a surface wind, be supplied by this zone; and that the winds experienced in the Polar regions are winds travelling on the surface, and are drawn from this belt of warm winds, is, I think, proved by the following extract from an account of the wintering of the *Heceta*, Capt. (Sir Edward) Parry, at Melville Island, in the year 1819-20:

"A gale of wind, from whatever quarter it might blow, was almost invariably found to raise the thermometer several degrees, even when it came from the north, as much as 14° . An east, south-east, or east-south-east wind causes the thermometer to rise 40° ."

From this extract it is evident, as might be supposed, that any current of air from this zone or belt of warm westerly winds raised the thermometer considerably, but that the wind that proceeded the more directly from the ocean and had the least land to traverse was the warmest.

Maury, without attempting to prove his case, and indeed throwing the onus of proof on those who ventured to disagree with him, considers that the south-east trade winds of the southern hemisphere become the south-west winds of the north temperate zone, and *vice versa*; that the north-east trades of the northern hemisphere become the north-west winds of the south temperate zone. I do not say that this is not the case, but if you admit that the north-east and south-east trade winds meet in the belt of equatorial calms and there ascend, it appears to be more reasonable to suppose that their currents intermingle and that their mixed volume is then drawn off north and south as required to restore the equilibrium of the atmosphere. And there is a very strong argument against Maury's hypothesis, viz., that as the south-east trades of the southern hemisphere are stronger and extend over a greater surface than the north-east trades of the northern hemisphere, and as also the north-west winds of the southern hemisphere are stronger and more continuous than the south-west winds of the northern hemisphere, it is illogical to suppose that the stronger polar current, i.e., the south-east trade, feeds the weaker equatorial current, i.e., the south-west winds of the north temperate zone; it would be more reasonable to suppose the reverse to be the case.

This entire theory of atmospheric currents is antagonistic to the presumption that a larger body of vapour is carried from the northern to the southern hemisphere.

Owing to the scarcity of land, and especially of very high land in the south temperate zone, not only is the precipitation less, but the vapour-carrying winds, i.e., the westerly winds, are far more constant in their direction and force than are the westerly winds of the northern hemisphere. (The proportion of westerly winds to any others in the temperate zone in the North Atlantic is two to one, while throughout the south temperate zone they are so constant as to have been christened by Maury the north-west trades.)

If you once admit that these westerly winds are equatorial currents flowing towards the poles (a fact susceptible of undoubted proof), it is easy enough to account for the low barometer in the Antarctic regions, as also the larger amount of precipitation there as compared with the precipitation in the Arctic regions.

1. Because the westerly winds being much stronger and more continuous in the southern than in the northern hemisphere, the ascension of the air in the South Polar regions must be greater than in the North Polar regions.

2. Because, owing to the westerly winds of the south temperate zone parting with less of their moisture (as previously accounted for) than the corresponding winds of the north temperate zone, and also to their being stronger and more continuous, it is evident that when they meet with Antarctic cold and their vapour is condensed, the precipitation must be greater, which also involves the giving out of a much larger

amount of latent heat and the consequent greater expansion and ascension of the atmosphere in the South Polar regions.

It is, I believe, universally acknowledged that all winds must blow from a high to a low barometer, i.e., from a zone of high pressure to a zone of low pressure (not directly, but in a direction modified by the earth's rotatory motion). I may therefore fairly argue that the zones of low pressure at both the equator and the poles proceed from the same causes, z.e., from precipitation, and from the ascension of the atmosphere, and that the lower barometer in the South Polar regions fairly accounts for the greater strength and continuity of the westerly winds of the south temperate zone, and that without these constant inequalities of pressure we should have neither trades nor westerly winds.

I have purposely from want of space avoided speaking otherwise than generally of the effect of the land on atmospheric currents, nor is it directly pertinent to my present argument.

The hypothesis of atmospheric circulation which I have very briefly sketched is in many of its features susceptible of absolute proof, more especially in the following points, viz.—

1. That the trade-winds descend to the surface of the ocean on the equatorial sides of the calms of Cancer and of Capricorn.

2. That the trade-winds ascend in the belt of equatorial calms.

3. That currents flow from the equator in the upper regions of the atmosphere in an opposite direction to that of the trade-winds on the surface of the ocean.

4. That these upper currents, flowing from the equator, descend again to the surface of the ocean on the polar sides of the calms of Cancer and of Capricorn.

5. That these equatorial currents, subsequent to their descent on the polar sides of the calms of Cancer and of Capricorn, are known as the westerly winds of the temperate zones.

And with reference to my supposition that these westerly winds ascend in the Polar regions, one strong evidence in favour of this is, that if, as I say, the ascension of the atmosphere is greater in the South Polar than in the North Polar regions, the counter or return current towards the equator must also be greater, which is the fact.

The onus lies on the promoters of the new hypothesis either to reconcile their views with the existing theory of atmospheric circulation or to supply us with a better theory, and one which shall agree equally well with well-established facts.

October 27

DIGBY MURRAY

Definiteness and Accuracy

In my lecture on *Force* (ante, Sept. 21), I take for granted that the scientific use of the word is that with which all are familiar in the expression "*the parallelogram of forces*." Hence Newton's term for *force* is *vis impressa* (Thomson and Tait's *Nat. Phil.*, § 217); though, where there is no room for mistake, he often employs the single word *vis*.

One of the main objects of my lecture was to protest against the absurd custom of translating the word *vis* in every case by the scientific word *force*. It is not easy to get an unobjectionable single word for the purpose, for most of the available words have already a semi-scientific sense attached to them. The word *power* is very flexible in its meaning, and would have been suitable had it not been already seized by the engineers. Thus (Thomson and Tait, § 216) *vis insita* is rendered *innate power*. And, giving the word as wide an application as Newton gives to *vis*, we might render *vis viva* as *active power*, which is not far from *actual* or *kinetic energy*. But this is merely a suggestion.

In Poggendorff's *Annalen* (No. 7 of this year) Prof. Zöllner translates the scientific term, "*the perpetual motion*," by "*die be-harrliche Bewegung*," and thus, to his own satisfaction at least, proves me to be ignorant alike of the proper meaning of the Latin *perpetuum mobile* and of the first law of motion!

In another journal I have lately been held up to scorn, not in the main for any real or imputed fault of mine, but because my would-be critic (Mr. R. A. Proctor) happens not to know the scientific meaning of "*absolute*" measure !!

I could give many more telling instances, great and small, but I have given enough to show how needful was my contention for definiteness and accuracy.

P. G. TAIT

College, Edinburgh, November 11